ment. It is thus impossible to discuss the proper value of P_0, as Prof. Harkness wishes to do, without raising the question of fallible observations. If it is raised, the method of treatment by least squares follows.

Prof. Harkness tried to show that, although the second term which I introduced brought the approximate value of P_0 nearer to that given by the ordinary formula, it removed it further from another value which he regarded as the standard. I venture to think that I have justified my position by showing that the introduction of P_0 is useless unless the equations are regarded as fallible; that the ordinary value is that given by least squares, and that the standards proposed by Prof. Harkness are founded on assumptions which have no theoretical basis.

In conclusion I may perhaps be allowed to make two remarks, one of which would, I think, from the point of view assumed by Prof. Harkness have strengthened his case. In the first place he is wrong in saying that the ordinary value of P_0 lies between P and P_1. It is smaller than both of them if A is $> A_1$.

In the next place I may point out that by treating a number of fallible expressions of the type of equations (1) and (2) by the method of least squares, a general value of L could be found without the introduction of the small theoretical errors which have caused this correspondence. There is however little doubt that by the introduction of P_0 we obtain a more convenient and practically no less accurate method of dealing with the observations. ARTHUR W. RÜCKER.

Science Schools, South Kensington, January 10.

The Mist-Bow.

IN a letter to the *Times* of January 12, Prof. Tyndall calls attention to a white mist bow, which he has seen on one or two occasions, and mentions its rarity of occurrence. It may therefore be of interest to record that I witnessed a similar phenomenon on January 9 last. My point of view was an elevated band-stand at the head of Weymouth Pier; the time 11 a.m. The air, as on the occasions mentioned by Prof. Tyndall, "swarmed with minute aqueous particles," *i.e.* was foggy, and on looking away from the sun, which was shining weakly, I saw a well-defined white bow cast upon the mist. The bow appeared to be about 60 feet distant. My point of view being high, a full semicircle was visible. It was, as may be imagined, a beautiful and graceful object. ALBERT BONUS.

St. Leonards, Exeter, January 13.

IN reference to Dr. Tyndall's letter in the *Times* of Thursday last upon the ullao as observed by him, I beg to call your attention to my paper read before the Stockport Society of Naturalists upon the same subject (see pp. 11 and 35). Not having seen the phenomenon described before, I ventured to call it the dew-bow. THOMAS KAY.

Moorfield, Stockport, January 14.

THE character and persistence of the recent fog have been so exceptional that perhaps you may deem the following observations on it worthy a record in NATURE.

I was staying in Mid-Devon at a place in the valley of the River Taw, some 10 miles north of Dartmoor. On Monday, the 9th instant, we were enveloped in a dense, damp, white fog, a rare occurrence in that part of the country. Surmising that the fog had no great vertical thickness, I sallied forth in the afternoon to mount a hill immediately to the eastward. At a slight elevation the sun was already making his appearance, and as I continued my ascent, and the fog became more and more thin, I saw before me on the then pale blue sky a beautiful white bow, similar to the rainbow, only broader and without colour. When the top of the hill was reached, the fog and bow had disappeared, the sky was deep blue, and the sun shining with quite spring-like warmth.

The scene I now had around me was most enchanting. The fog could be traced lying in the river valleys like arms of the sea, with the bordering hills simulating cliffs, and here and there an island appearing in the midst, whilst the distant Dartmoor hills stood out calm in unbroken sunshine. No movement of the air could be detected, but, below, the surface of the fog seemed as if being rolled along by a wind from the east towards the river valley. The white fog-bow is seldom seen, and I imagine owes its absence of colour to the minuteness and close proximity of the water globules, allowing the divided rays to coalesce and so again form white light. C. O. BUDD.

Atmospheric Effects at Sunset.

ON Sunday, January 8, upon leaving the house at half-past four in the afternoon, I observed that the clouds were suffused with a kind of pink or lurid coppery tinge, a sort of angry sunset tint spread over the whole sky. The clouds were of the stratus type which is common in a winter anticyclone, but were moving or rather driving with a swiftness quite unusual under such conditions. The barometer was very high and rising rapidly; but during the afternoon there were several violent and noisy gusts of wind almost amounting to squalls, though during the greater part of the day the atmosphere was still almost to stagnation. The air was mild and intensely humid, and everything was dripping with moisture. In fact the weather was in many particulars the opposite of what we expect during the prevalence of an anticyclone. The diffused sunset effects were quite unlike anything I ever remember to have witnessed before. The gas-lamps had just been lit, and the flames not only appeared of a greenish tint, but seemed to be inclosed in green glass. Several persons stopped me in the street and inquired what it all meant, and one acquaintance said, "What is going to happen?" In the green tint of the gas there is, of course, some suggestion of a colour complementary to the strange red glow which seemed to pervade the atmosphere. But in the absence of all, even the most rudimentary, knowledge of the subject, I should be glad if you or some of your readers can explain the cause to me and to others who witnessed the unaccustomed phenomenon.

CHARLES CROFT.

Prestwich, near Manchester, January 9.

Newton's "Principia."

IT may perhaps interest your readers to know that the 200th anniversary of the publication of Newton's "Principia" was solemnly celebrated on December 20 (old style) by a united meeting of two learned Societies of Moscow—the Imperial Society of Friends of Natural Knowledge, and the Mathematical Society. Prof. Mendeléeff, of St. Petersburg, was Honorary President. Prof. Stoletow (President of the Physical Section in the first-named Society) presented a sketch of Newton's life, and spoke on his optical discoveries; Prof. Zinger (President of the Mathematical Society) treated Newton's mathematical work; Prof. Joukowski pointed out his merits as founder of rational dynamics; and Prof. Ceraski exhibited the creation of celestial mechanics by Newton. The large hall of the Polytechnic Museum, where the meeting took place, was attended by the *élite* of the city. The lectures were illustrated by some optical experiments with electric light and some lantern-slides relative to Newton's biography. A. STOLETOW.

University of Moscow, December 21, 1887 (January 2, 1888).

Meteors.

IN the moonlight on the evening of January 2, at 10h. 58m., a fine meteor, equal in brightness to Jupiter, was observed by Mr. D. Booth at Leeds, and by myself at Bristol. As seen from Leeds, the meteor passed from Musca to the head of Cetus, and terminated its course about 3° east of α Ceti. It moved rather quickly, leaving a long thin train. The fore-part of the nucleus was tinted with red, but the train was yellow. At the finish the motion became slower. At Bristol the meteor was first seen when about 6° S.E. of ζ Draconis, and it travelled some 8° in the direction of β Cephei. Colour yellow, motion very slow. The course was evidently much foreshortened close to its radiant.

Comparing the two paths, it will be found that they intersect each other at 250° + 57°, so that the meteor was not a member of the January Quadrantids, which attain a maximum on January 2, but belonged to a neighbouring shower of Draconids, which, between January 14 and 19, I have previously observed at 253° + 56°. The meteor appears to have been observed earlier in its flight at Bristol than at Leeds, for at the latter place the observer was watching the southern sky, and only caught the later part of the course. From a mean of the two observations the height at commencement was 98 miles above a point west of Appleby, Westmoreland, and the end occurred at 60 miles above Chester. The earth-point was near Tiverton, in Devonshire. The real length of path was 109 miles, and it was inclined at an angle of 20½° to the horizon. The meteor was travelling in a direction from north to south, the bearing of the radiant being N. 8½° E.

It would be interesting to hear of further observations of this bright meteor. It must have been seen by many persons, as the night was very clear.

The fireball of February 21, 1865, had a radiant at 255° + 55°, and close to that of the meteor of January 2 last, but the difference of date is too considerable to permit an inference that the wo bodies diverged from the same stream.

January 8. W. F. DENNING.

IN NATURE, November 10, p. 36, it is stated in reference to a meteor that "a Norwegian astronomer" is of the opinion that the track of the meteor must have lain too high to be heard. "He calculates from the reports to hand that the bursting of the meteor occurred at an altitude of about 6000 feet (*sic*), and he thinks that even this figure may be safely doubled."

It may interest some of your readers to know that on the night of July 3, 1884, at 8.27 p.m. standard time of the 75th meridian, a meteor was seen by me, as well as by others, here, and about 5m. 17s. afterwards a sound was heard something like distant thunder, except that it seemed to swell rapidly and steadily to a maximum intensity, and then diminish again in much the same way, but more slowly. I immediately connected the sound with the appearance of the meteor, and stated that it must have been a little over sixty miles distant, and from the estimated angle of elevation about *thirty miles* above the surface of the earth. This estimate was borne out by the accounts from other places of the course of the meteor. The sound I should be inclined to attribute to the rushing together of the air in the wake of the meteor, or perhaps more probably to the sudden compression of the air in front of it, and not to its bursting.

The following account of the meteor was given in the *Canadian Weather Review* of July 1884:—"A magnificent meteor was seen on the night of the 3rd at 8.27 p.m. standard time, passing from south-east to north-west, colours brilliant red and green. Two distinct explosions are reported to have been heard. After the first explosion a sinuous streak remained visible until covered by clouds; the time of flight was from seven to eight seconds, and the apparent size about one-fourth that of the moon. Reports have been received from Listowel, Hastings, Beatrice, Belleville, Lakefield, Pembroke, Peterborough, Kingston, Deseronto, Lindsay, and Huntingdon, all substantially agreeing as to course, size, &c.; it passed two or three miles south of Belleville, and about the same distance north of Lindsay." CHARLES CARPMAEL.

Toronto, December 16, 1887.

The Electrification of the Air.

IN writing upon the electrical condition of the Peak of Teneriffe, the Hon. Ralph Abercrombie (NATURE, vol. xxxvii. p. 31), begins by stating that "the limited number of observations on atmospheric electricity which have been already made all point, with one exception, to a normal positive difference of potential between a point some few feet above the earth and the ground itself;" and farther on he writes: "the electrical conditions of the Peak of Teneriffe [the one exception] were the same as in every other part of the world." As similar statements still find their way into text-books and treatises on electricity and meteorology, I trust you will permit me to point out that, unless a very special meaning be attached to the word "normal," this generalization is decidedly too wide.

In a paper read at the Aberdeen meeting of the British Association in 1885 (printed *Phil. Mag.*, November 1885), I pointed out that, in Madras at least, a negative electrification of the air was a normal, and not an abnormal, condition for many hours of the day at certain seasons of the year. Observations since taken have entirely confirmed the opinion that with a hot, dry, west wind the air at Madras is usually negatively electrified, and often to a very high potential.

With regard to observations made on mountains in the tropics, though perhaps hardly within what Mr. Abercromby terms "the zone of constant electrical discharge," I would venture to call his attention to a short paper on observations made on the top of Dodabetta (8642 feet) in the Transactions of the Royal Society of Edinburgh, vol. xxxii. p. 583.

I may add that during the periods of incessant discharges of sheet lightning which we often experience here the electrification of the air is sometimes positive and at other times negative, but generally positive. C. MICHIE SMITH.

Madras Christian College, Madras, December 14, 1887.

Wind Force at Sea.

PROF. WALDO, in the *American Meteorological Journal* for October, recommends the use of instruments for determining the velocity of the wind at sea. In a paper read before the Meteorological Society, I discussed the comparative results, obtained from a great number of observations under all conditions at sea, between two very simple and small anemometers, showing that, although the two instruments were on entirely different principles, the results obtained differed only by about 10 per cent. In a paper read in March last before the Meteorological Society, "Notes on taking Observations at Sea, &c.," I again urged the desirability of observers using some form of anemometer, so that more uniform results could be obtained, and I gave a table for correcting the apparent velocity of the wind as registered by the instrument for the speed of the ship and for aberration.

For instance, at the present time you may have two sailing-ships close together, one carrying top-gallant-sails, the other only reefed top-sails, and the wind will be logged accordingly. Again, two steamers going in opposite directions are very likely to experience apparently different wind velocities, and the senses of officers in steamers are not so acute for detecting differences in wind velocities as are those of officers in sailing-ships. The use of instruments would eliminate these errors.

With instruments similar to those I use—the coefficient of friction of which is slight—the relative velocity of the wind may be obtained fairly accurately; and I contend that this is of more importance than the chance there is of obtaining the estimated true velocity; and, I may add, the trouble attending the use of these instruments is small.

There are two other subjects which, up to the present, have received little attention at sea, viz. the registration of rainfall and the electrical condition of the atmosphere. Observations on both could easily be carried out on board some ships, and the observations would be both valuable and interesting.

DAVID WILSON-BARKER.

A Troublesome Parasite of a Brittle-Starfish.

IN a valuable work on certain parasitic Crustacea ("Contributions à l'Etude des Bopyriens," p. 181), Prof. A. Giard and J. Bonnier have done me the honour of calling attention to my discovery of a Copepod (?) which lives in the body of an Ophiuran, *Amphiura squamata*. They regard the mutual relationship of the Copepod and the Ophiuran as an instance of the castration of the host by the parasite. Although all my observations indicate the correctness of some such an interpretation, I failed to recognize it as a fact until after they had pointed it out. The explanation seems a possible one, and is provisionally accepted, with a few modifications, as the best as far as research has gone. The modifications are important.

The state of knowledge of the subject is as follows. Ova and young of a Crustacean are found in the body of an American brittle-star, identified as *Amphiura squamata*. In some instances an adult Crustacean was also found in the same place. When these ova, young, or adults are found parasitic in the Amphiura, the remains of the ovary of the host appear as an amorphous mass, and there is no possibility of future young of the Amphiura in the brood sac, since the ova have been destroyed.

The conclusion seems inevitable, for observations indicate that the mother Crustacean makes her way somehow into the body of the host (Amphiura), then affects the brittle-star so that the young of the host will not develop, after which she leaves packets of ova to mature in the sacs where normally young Amphiuræ would develop. It thus happens that the products of the ovary of the host are destroyed before the Crustacean ova are developed, or while they are in an early stage of cleavage. Consequently it is legitimate to conclude that if the ova of the host is destroyed it may be done by the adult Crustacean.

If Prof. Giard and Bonnier are right in their interpretation that this is an instance of parasitic castration, as I think they are, we possibly have an interesting case of a parasite destroying the reproductive powers of the host *for the future good of her own offspring*. Such a condition of things is unique, and among Ophiurans the writer recalls but the single instance of the present case of Amphiura. The case of the Crustacean and its brittle star host seems to differ from that of Entoniscus in that in the one instance the destruction of the ovary may be of advantage to the parasite, while in the other the destruction or